

observations used a micrometer with one wire bisecting the disc, presumably set so as to indicate the measured half-diameter of the planet, so that Barnard's *central meridian* can hardly have differed in the sense implied. Concerning the direct measures of the distance between the two spots Professor Barnard writes that "although the spots were usually very conspicuous, it was found that they were quite ill-defined and rather difficult when the wires were placed over them."* This statement seems to support the suggestion made above as to the possible prejudicial effect of a micrometer wire in altering the appearance of a spot. Without a micrometer these spots were almost ideal objects for observation.

Hove: 1904 November 29.

On the Eclipse of Agathocles. By Simon Newcomb.

In my "Researches on the Motion of the Moon," which appeared in 1878, I made a careful study of the accounts of supposed total eclipses of the Sun by ancient authors, with a view of determining whether any of them could be used either as tests of the lunar tables, or as auxiliaries in the determination of the slow changes in the lunar elements. The conditions required for this use were that some determinable eclipse should have been total at a known place. The conclusion which I reached was strongly in the negative. Not only did there appear to be no ancient eclipse which we could conclude was really total at a given place; but the accounts were generally so vague that no interest seemed to attach even to a comparison with the lunar tables, except for chronological or historical purposes.

I may add, in all frankness, that these adverse views have not been shared by those astronomers who, in the meantime, have made researches on the subject. Oppolzer and, after him, Ginzl had such confidence in the reality of these eclipses as to use them as the basis of corrections to the elements of the Moon's motion which are incompatible with gravitational theory. I have no intention to argue my view at present further than to say that I am not at all convinced it was in any point ill founded, so far as related to data available at the time. But since my paper was published a very important point has been brought to light showing an exception to the conclusions there reached. This arises in connection with the eclipse of Agathocles — 309 August 14.

This eclipse has been so fully discussed by Airy and others that only a very brief statement of the circumstances connected

* *Monthly Notices*, vol. li. p. 549.

with it is necessary. The eclipse was observed from the fleet of Agathocles about 10 o'clock A.M. of the day after it put to sea from Syracuse. There is no doubt about the fleet having been well within the line of totality. Unfortunately it is uncertain, from historical evidence, whether Agathocles sailed to the north or to the south of Sicily. Consequently there are two possible positions of his fleet, one to the south of the island, the other near its north-east point. Moreover, the path of totality was quite broad, the radius being about 50', thus much widening the range of uncertainty.

What gives real importance to this eclipse is its identification by Celoria with one referred to by Cleomedes, during which the Sun was said to have been entirely eclipsed in the Hellespont, while one fifth of its diameter was still visible at Alexandria. This affords a very strong presumption that the path of totality passed over the Hellespont. Yet the path does not reach the Hellespont by the tables either of Oppolzer or Ginzel, the central line being a hundred miles or more to the south. What I have now done is to make a computation of the path of totality from the tables based on the corrected theory found in the researches already alluded to, using the tables in the astronomical papers of the *American Ephemeris*, vol. i., as well as testing the result by a differential reduction of Hansen's results given in his "Darlegung." The result is that, according to my tables, the path of totality passed the following points, the one near Sicily, the other south of the Hellespont :

| | Central Line. | | | | Northern Limit. | | | |
|---------------|---------------|----|----|----|-----------------|----|----|----|
| Longitude ... | 15 | 35 | 23 | 39 | 15 | 35 | 23 | 39 |
| Latitude ... | 37 | 7 | 38 | 1 | 38 | 4 | 38 | 58 |

The extreme south-west end of the Hellespont is in latitude 40° , longitude 24° . According to my corrected theory, the northern limit of the path passed some miles south of this point, and, of course, yet farther south of other regions of the Hellespont. In order to bring the northern limit well into the Hellespont it would be necessary to make a yet farther diminution of the secular acceleration by about $1''.5$.

Both positions of Agathocles, as estimated by Airy, fall within the computed belt of totality, so that we cannot base any conclusion on the account of that navigator.

Very interesting in this connection is the conclusion reached by Mr. Nevill in his paper in the *Monthly Notices*, vol. xxxix. p. 72. Here the author argues vigorously against my introduction of a rather indefinitely described eclipse of the Moon used by Ptolemy ; and, indeed, of the series of Babylonian eclipses of the Moon which I used in my determination of the secular acceleration. My reply to this view would only have been that I fixed the weight to be assigned to these eclipses solely upon the expressions used by Ptolemy, and did not think it expedient to make

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any change on account of the observed discordance. Possibly this was not the best course in a case where so much suspicion may attach to a record. Mr. Nevill's conclusion was that my result should have been still further diminished by at least $1''$, and perhaps $1''\cdot5$. This view is now strengthened by the eclipse of Agathocles.

The important point is that this reduction will carry the observed acceleration down almost to the theoretical value, in which no allowance is made for tidal retardation. In other words, the conclusion to which the new evidence points is that the actual retardation of the Earth's rotation is almost evanescent. Although no numerical determination of the probable amount of retardation, as given by theory, has, so far as I know, ever been made, I think any estimate must make probable a value larger even than that corresponding to my former result. It therefore seems likely that a neutralisation of the effect of tidal friction is produced by some cause not yet fully investigated.